Biogeosciences Discussions

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Comment on bg-2022-203

Anonymous Referee #1

Referee comment on "Throughfall exclusion and fertilization effects on tropical dry forest tree plantations, a large-scale experiment" by German Vargas Gutiérrez et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-203-RC1, 2022

This manuscript aims to tackle one very important question related to how climate change will affect tropical (dry) forests: the actual role and interaction between water and nutrient availability controlling forest growth. The authors studied different aspects of aboveground and belowground plant/soil components in a factorial throughfall and fertilisation experiment in a tropical dry forest in Costa Rica. This is a first for tropical forests and this fact alone already grants the manuscript a high interest for the scientific community. Besides that, I believe that the manuscript is a good fit for this journal as well. Due to the high number of studied variables (which is great, of course) and the experimental design that has the effect of drought, nutrients, tree species, N-fixing capacity and canopy versus understory trees, some parts of the text get a bit hard to follow. Nonetheless, because of such complexity, I acknowledge that the authors did a very good job in reporting the results, both in terms of the text, but also figures/tables. Supplementary material also has some important extra data that made it easier to grasp some of the reported trends (or lack of). As you will see in my comments below, I have some technical small comments on writing (very minor) and some comments on some things that are not clear as they are in the text at the moment. I hope the authors find them useful to improve the manuscript.
In my opinion, one weakness is that although there are many non-significant or marginally non-significant results/trends, the authors still base a lot of the discussion and conclusions on those. I understand that there are not really a lot of strong responses, especially with drought, but I suggest then the authors tone down some statements, and for the purpose of improving future science, acknowledge and suggest where and how such an experiment could be better replicated in the future.

Line 21

In the abstract, I had the impression that some results that are later reported as trends but are not significant are emphasised here. Perhaps in this way, the abstract is overselling the story a bit.

Line 112-113

This reference could come in the introduction but also it could be relevant for some parts of the discussion of the results.

https://www.nature.com/articles/s41586-022-05085-2

Table S1

It’s not clear what’s the unit of the fertiliser values.

Table S1 and S2

It’s a minor detail but treatment abbreviation differs in the supplementary material and the main document.

Figure S4

It seems that the significant difference in LAI in 2017 was found in all treatments, but only DR and FR are discussed in figure caption. Can you clarify here, please?

Line 155/ Figure S2

Indicate somewhere how distant the plots are from each other.

Line 159

Any specific design for soil sampling (e.g. corners/centre of the plot)?

Line 175

Is there any indication that the study sites were more N or P limited? I see the focus of having comparable N concentrations (like the experiment in Panama), but I just wonder about the other elements, why the choice of a broad-spectrum fertiliser?

Line 185

I thought that due to its tree-centric approach in building the plots, only the planted trees were going to be taken into account when measuring productivity. Couldn’t the panels at the throughfall exclusion limit new plant appearance/recruitment?

Line 200-201

There is, perhaps, a mistake in this sentence, as “leaves, and, reproductive litterfall” appear twice in the description.

Line 238

I understand how difficult it must be to sample these cores in the dry season. One thing that needs clarification here then, is if root productivity in kg m-2 yr-1 was calculated extrapolating those 2-month interval sampling periods to one year, or if root productivity between, for example, November to June next year was taken into account. If the second is true, I would imagine that during these 6 months root productivity might not be accurate, as root mortality and recruitment could have happened. On the other hand, by using root productivity only during the wet/growth season, perhaps root productivity on a yearly basis could be overestimated (assuming there is higher root productivity in the wet and lower in the dry season). In year 1, you sampled along the whole year, right? How does the data between different seasons compare for this year?

Another point, so in every sampling, you installed new ingrowth cores in different locations. Did I get that right? If so, would you also have root stocks in those same sampling dates, or when freeing the soil from roots to install the ingrowth cores, these roots were discarded? Just curious to know a bit more about the root dynamics here.

Line 315-316

Could you please add to this sentence if such reported differences are statistically significant?

Lines 315-318

My main concern is related to the extent to which the throughfall exclusion really worked, and how this may have affected the general lack or weak trends you find in your study. This should be acknowledged a bit more in the discussion, perhaps in a way to provide advice for future research. Many of the results you show in the supplementary material, for example, regarding the production of flowers and seeds, are marginally non-significant, despite the big difference in productivity. I imagine that in addition to specific species differences, the relative short nature of the experiment was not enough to capture significant changes.

Line 321

Since there were not such big differences between mean soil moisture comparing control plots and throughfall exclusion, I wonder if this signal is small because of a potential effect of the years, meaning that perhaps stronger patterns could be seen towards the end versus the start of the experiment. Have you tested for the changes in soil moisture between years and treatments?

Line 330-331

Can you please add to this sentence if such interaction resulted in a positive or negative trend?

Line 870

I think it should read “median”, not “media” (or mean?). Same for some figures in the supplementary material.

Line 356

There is a discrepancy between the unit described for root productivity in the methods section (annual basis) and here in the results.

Line 381

You state there are no significant differences but the p value is 0.0431, can you clarify this please?

Lines 421-423

Great argumentation here!

Line 426

Again, I think that this recent paper could make it to the discussion (strong and direct evidence of P limitation in Amazon forests). This paper contradicts the statement you also make on line 500 in the conclusion, since the authors found strong NPP responses after 2 years of fertilisation in a mature forest.
https://www.nature.com/articles/s41586-022-05085-2

Lines 434-436

I would suggest toning down this sentence a bit, as this is a trend, and no real strong evidence of colimitation by water and nutrients were found in your study.

Line 441

Insert . after “treatments”.

Line 456

Better to use the past tense here: “experienced”.

Line 463

Could you specify which resources you refer to here, perhaps light?

Line 475

It could be useful to acknowledge the fact that in these dry forests, and especially by increasing drought experimentally, roots can go really deep/deeper in search for water and nutrients. The lack of responses found for root productivity in your study is limited to

the 0-15cm, and if we think that you only captured changes in soil moisture at the 40cm depth, it’s plausible that roots could be changing down the soil profile as well.

Line 490

Maybe replace ; by , after “disturbed soil”.

Line 498

It seems there is some word missing between “responses to” and “is sensitive to”.